SAVING SCIENCE FROM SCIENCE STUDIES: A CONSTITUTIONAL ECONOMICS APPROACH

Christoph Luetge
University of Munich

Abstract

Scientific rationality has been severely questioned by science studies. Some philosophers of science, esp. Philip Kitcher, have been using economic arguments to counter this attack. But their conceptions left open flanks for standard objections against economics. In this paper, constitutional economics will be presented as an alternative conception that could remedy some deficits of economic approaches to philosophy of science. In particular, consensus is regarded as a normative basis, and the analysis of dilemma situations followed by the design of an adequate institutional framework is seen as an important task of economics and also of philosophy of science. Some examples of this are sketched.

Introduction

The science wars rage on. The battle between contestants and defenders of (traditional) scientific rationality is not likely to be over soon [cf. Gross and Levitt 1994, Ross 1996]. Science studies, sociology of scientific knowledge, and other branches are still fiercely opposed to followers of rather traditional methodology (philosophers and scientists) on the question whether (and if yes, on what grounds) science can be called a rational enterprise.[1]

Yet there is a new development. The defenders have in recent years found a new weapon: Economics. Philip Kitcher, Wade Hands, Nicholas Rescher, James Wible, and others have been using methods and results from economics for some time already.[2] Some of this work, especially Kitcher's,[3] explicitly turns economics against science studies. I will discuss his argument in section 1. Of course, the opponents have not remained mute, but have heavily criticized Kitcher's approach. Some of their main points will be discussed in section 2. Afterwards, I will propose a way of dealing with these criticisms. Basically, the idea is to bring in a different version of economics: constitutional economics. I will outline this research program in section 3, before discussing some of its possible applications to philosophy of science in section 4.

1) The economic argument for scientific rationality: The invisible hand

Economics is, of course, not the only 'social' approach to understanding science. The most important rival approaches are sociology of science and sociology of scientific knowledge.[4] These differ from economics in several ways. The most important difference seems to be that sociologists mostly emphasize the epistemic *deficiencies* of scientists and point to *inadequate* scientific institutions.[5] They mainly show how knowledge is being *distorted* by interests. By contrast, economics can view science from a different angle: Economics could help show that science can, after all, deliver relatively reliable knowledge *despite or even due to* all its sociological inadequacies. There are at least two aspects of this idea:

1) First, economists can reconstruct social institutions as *rational* solutions to problems of social interaction. The classic example from the history of philosophy is Hobbes' argument for the social contract: A social contract to form a state primarily solves the problem of how rational actors can *rely* on each other's actions. The actors agree on rules, on institutions and also on ways of enforcing these rules. Without a social contract – in an anarchic state, i.e. without enforcement of rules –, there would be incentives to exploit others: to steal, to plunder, to kill. No actor, strong or weak, profits from such a situation in the long run. Therefore it is rational for them to consent to a social contract as a win-win-solution, holding advantages for all involved. Economics reconstructs many institutions in this way,[6] which should be used on science as well. Scientific institutions can also be reconstructed as rational solutions to problems of social interaction.

2) Second, economics deals with many problems of interaction in which each actor serves his own interest while *at the same time* serving the interests of others. Situations like this should be looked for in the analysis of science as well. So while sociologists of science often talk about self-interested scientists, they do not systematically allow for the possibility that despite this individual self-interest, the overall outcome might be rational for all. Here again, economics can provide arguments for the rationality of science despite – and even due to – the social character of science.

Philip Kitcher has especially been focusing on this second idea. His work [Kitcher 1993, cf. also Kitcher 1990] systematically links scientific rationality to the self-interested of researchers.[7] His main task in "The Advancement of Science" [Kitcher 1993] is to defend philosophy of science against relativism, especially from sociology of science. For this purpose, he employs, without explicitly using the term, the classic economic argument of the invisible hand.[8] On the one hand, he accepts the influence of social factors on scientific knowledge. On the other hand, he maintains

that social aspects do not necessarily obstruct scientific progress, but might – under certain conditions – promote it. As in Adam Smith's classical argument, Kitcher holds the view that the positive consequences of distribution of labor can be found in science as well [cf. already Kitcher 1990]. Under specific constraints scientific progress is fostered, not hindered by self-interest.[9]

Kitcher tries to make this idea more plausible by using formal models from economics:[10] He models the operation of social systems in a way that is a standard procedure in economics: Given certain assumptions about a) the actors'preferences, b) the constraints of the situation and c) the behavior of the agents according to the rationality principle, he tries to show that there exists some equilibrium point to which the interaction process converges. This does not mean that this point will necessarily be reached in reality, but it is a *possibility*.

These models have been the target of critics [cf. Hands 1995, 1996 and 1997; Mirowski 1996], which I will turn to in the following section. Later, I will argue that the invisible hand argument does not work by itself, but has to be embedded in a methodologically sound conception of economics – constitutional economics – in order to counter the criticisms which, in the end, boil down to standard objections against economics.

2) Critiques of the economic argument

Kitcher's use of economics in philosophy of science has been criticized heavily.[11] In particular, his economic basis (mainly neoclassical economics) was questioned for its own unresolved methodological problems [cf. Hands 1995, Mirowski 1996]. For example, Kitcher has been criticized for uncritically speaking of "consensus practices" in science [cf. Hands 1995]. But economics is (usually) firmly rooted in a tradition of methodological individualism. So in what sense can common practices or goals be identified? Kitcher does not provide a systematic answer to this problem.[12]

More specifically, Mirowski [1996, p.160] has been criticizing Kitcher for dreaming of a "generic aggregate 'welfare function'", the same dream that haunted "nineteenth century British utilitarians and their twentieth century cousins, the neoclassical economists."[13] Mirowski compares Kitcher's view of non-relativist scientific goals to the welfare economists'search for a social welfare function.[14] According to Mirowski, both dreams are futile.[15]

But these are standard criticisms concerning the economic tradition. And this tradi-

tion already has ways of countering these criticisms. There is a direction of economic thinking, James Buchanan's "constitutional economics", which has hitherto very rarely been applied in philosophy of science[16] but which could be a very useful tool in avoiding many of the difficulties of other economic approaches.

3) Constitutional economics

Constitutional economics, sometimes also called the Buchanan research program, is based on James M. Buchanan's writings [cf. especially Buchanan 1975 and Brennan/Buchanan 1985]. This economic approach is directed mainly against the central ideas of welfare economics, especially against the existence of a social welfare function. Constitutional economics aims at a fundamental change in economic theory along the following four points:

1) Economics should rather be seen as the science of costs and benefits quite generally, than as the science of market processes. It does not deal with material goods alone, but with many different sorts of costs of benefits: Elements like psychological or intellectual costs can be integrated into utility functions. Economics thus aims at a universal theory of human behavior.

2) Economics deals primarily with interactions, especially with the analysis of dilemma situations (such as the prisoners' dilemma), and only secondly with individual decision-making.[17] Many traditional textbooks in economics start with the problems of Robinson Crusoe on a deserted island. However, constitutional economics stresses that economics deals primarily with *interactions*, with problems that arise in the social relations of human beings. This does not mean that there are no economic problems for an isolated actor like Robinson Crusoe. Certainly he has to cope with scarce resources. But these problems are problems of *action theory*, which models individual actions and decisions using the analytical tool *homo oeconomicus*. By contrast, there are no conflicts of interests and no *problems of interdependence* of actions on Crusoe's island. These questions only become crucial in multiple agent economies and other social subsystems (like scientific communities). The economic tools useful for Crusoe's world are inadequate for the economic problems of social systems.

Of course, we still need action theory: Models of interactions are *based on* action-theoretical models. In order to explain social phenomena, first the actors' situation has to be analyzed (step 1). This results in finding relevant *constraints* of the actors' situation. Second, the individual decisions and actions of the actors have to be modelled (step 2). Finally, these individual decisions are combined, often in matrices

of game theory (step 3). Step 2 is part of action theory, step 3 belongs to interaction theory. Coleman [1990, p.10] has illustrated this method with his 'bathtub model':

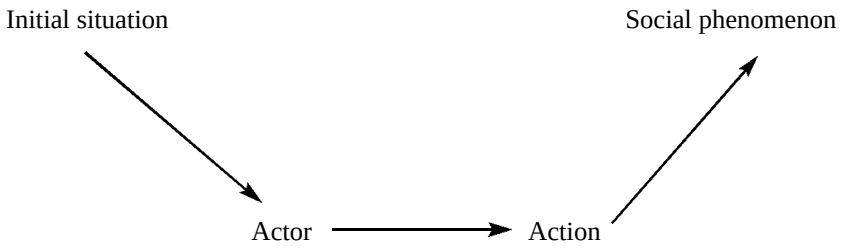

Fig. 1: Coleman's 'bathtub'

3) The main purpose of economics according to constitutional economics is not just a theoretical, but a practical one. Institutions (e.g., a market order) have to be designed as the framework for decisions of individual actors. It is only in view of this goal that it makes sense to model individuals – including scientists – as self-interested actors. *Homo oeconomicus* is only to be taken as a *construction* for a specific purpose: Institutional design. Other models of man – e.g., sociological or psychological ones – might be better suited for other purposes. In this way, critics calling for more realism in economic models can be answered.

4) There are no external normative criteria. Instead, normativity has to be grounded in consensus. In particular, normativity can be based on a hierarchy of levels of consensus. The application of this idea to philosophy of science will be elaborated in the following section.

Constitutional economics thus opens up an interactionist perspective on economic phenomena. I will now discuss some of the possible applications to philosophy of science.

4) Applying constitutional economics to philosophy of science

4.1) Consensus in science

Constitutional economics can shed a new light on the normative foundation for methodogical rules, one of the key problems in economic approaches to philosophy like Kitcher's who ground methodological criteria in some form of consensus.

Constitutional economics can be useful here. It takes the consensus of all citizens to be the only adequate normative basis for the political rules governing modern societies. If a solution for a political or economic problem is proposed, it must be possible in principle to reconstruct this solution as being legitimated by the consensus of all involved. However, trying to come to a consensus concerning every single problem would be too costly. Therefore, it is only required that the most abstract principles of a constitution are consented to. These principles provide rules for the process of deciding problems of lower levels. This results in a hierarchy of different levels of consensus (like: simple majority, two third majority, unanimity). For example, a society will usually not come to a consensus on detailed tax rules. But constitutional constraints on the fiscal process can more easily be agreed on, mainly because it is much more uncertain how each individual will be affected by these constraints [cf. Buchanan 1975, p.47].

A similar reconstruction can be applied to science as well. Science is governed by rules legitimated by a hierarchy of more abstract rules and ultimately by consensus. On the *lower* levels of this hierarchy, unanimous consent to rules is not required. This applies to *external* scientific rules (such as laws governing universities) as well as to *internal* scientific rules (such as classical methodological criteria like internal consistency, explanative value etc.). Moreover, on these lower levels, decision rules can differ significantly from those in other social subsystems: *External* rules are subject to the usual procedures of administration and legislation in the same way as, e.g., traffic rules. But *internal* rules are not subject to majority votes. Instead, internal rules are often shaped by only a small fraction of a scientific community's members. A small elite usually takes the lead in methodological questions, while the other members consent to these decisions only *ex post*.[18] I will give an example of this in the next section.

On the basis of this economic reconstruction of scientific normativity, Kitcher's term "consensus practices" (and Laudan's term "our goals") can both be given a new meaning. According to Hands [1996, p.145], legitimate collective goals cannot easily be grounded in a consistent naturalistic – and thus economic – conception. But according to constitutional economics, there *is* a naturalistic criterion of normativity: Consensus, more precisely a consensus of several different levels. Drawing the analogy to Buchanan's work: The most abstract principle that the members of a society consent to is the existence of *any* state instead of no state. The Hobbesian anarchy[19] is always the worst option for anyone.

When applied to science, this means: There should be a very broad consensus that

an *anarchic* situation in science is unacceptable. Science needs at least a minimum of universally accepted rules. In an economic perspective, this solution is still superior – even if rules are, e.g., selected at random – to one in which everyone observes only his own rules and in which no one is 'forced'(e.g. in academic training) to adopt *some* common rules.[20] Economics points to the fact that in a world of limited resources, only a few theories will be able to gain enough resources for systematic scientific investigation. Anarchy leads to an undesirable waste of resources, while pluralism allows – for example – for a dominant and several dissenting, alternative theories.[21] Moreover, methodological anarchism would not lead to pluralism and toleration:

> And unless some modicum of agreement is enforced, even those areas within which anarchy might indeed suffice to generate tolerable order would be subject to gross violations. [...] [A]narchy works only to the extent that limits among persons are either implicitly accepted by all or are imposed and enforced by some authority. [Buchanan 1975, 8f.]

So in a situation of methodological anarchy, scientists would either implicitly, perhaps due to a common ideology, accept certain rules, or the (seemingly) anarchic situation would lead to the uncritical (and dogmatic) adoption of certain rules.

Taking the analogy to the political area further, not only a rather *broad* consensus on the most abstract principles of science, but also a *hierarchy* of descending levels of consensus could be reconstructed: First, the members of a society consent to leaving methodological decisions to scientists themselves. Second, scientists consent to leaving these methodological decisions to a small elite. Thus, methodological rules are legitimated by a hierarchy of levels of consensus. I will try to at least sketch two applications of this idea:

a) Kitcher [1993, p.217] builds on Rudwick's analysis of the Devonian Controversy[22]. Here, the members of the (British) society *first* consented to leaving methodological decisions to the geologists scientists themselves (though external rules of funding, e.g., were still subject to political decision-making). *Second*, all (important and non-important) geologists consented to leaving methodological decisions to a small elite of only about ten people. Their authority in these questions was not in doubt during the controversy [cf. Rudwick 1985, 420ff.]. So in this case, it was just ten leading geologists deciding the adoption of the new Devonian system for their entire discipline. Rudwick clearly shows that all less prominent geologists accepted the leading role of the elite. In 'exchange', these minor geologists – rationally – claimed to be regarded as competent in details of local geology.

b) Wesley Salmon has provided a good example for different levels of consensus concerning scientific explanations. In his "Four Decades of Scientific Explanation", he states four tenets which nearly all philosophers of science nowadays consent to [cf. Salmon 1989, 180f.]:

1) Science provides explanations, not just descriptions. Up to the forties and even the fifties explanations were regarded as part of theology or metaphysics.

2) Logical empiricism fails. In particular, probabilistic explanations must run along different lines. Salmon admits that there is less consent to No. 2 than to No. 1.

3) Hempel's and Oppenheim's D-N-model has the virtue of formal rigor, but it is not very fruitful when applied to the actual problems of scientific explanation.

4) The pragmatics of scientific explanations are very important. Traditional philosophy of science was wrong to neglect them.

Below this level consensus can – according to Salmon – be found only within three different schools of thought: the pragmatist, the deductivist, and the mechanist approach.

Reconstructions like this might be extended to other areas of philosophy of science. Though originally made for other purposes, they can be interpreted as being compatible with the normative basis of constitutional economics: There is a hierarchy of different levels of consensus among the members of a scientific community (in Salmon's case, the philosophers of science).

Buchanan's consensus model thus presents an answer to the problem of how to get from the individual to the social level. Trying to find a social welfare function is futile. Instead, a consensus of individuals should be reconstructed which does not extend, however, to common goals but to common process rules or decision mechanisms. Kitcher's "consensus practices" can thus be reconstructed in a constitutional economics way: Consensus practices are grounded in a consensus of all people involved. However, these individuals consent only to some very abstract rules which do not strictly determine, but *guide* the decisions concerning methodological rules of lower levels and eventually particular consensus practices.

4.2 Dilemma situations in science

Beyond the general consensus perspective on scientific normativity, constitutional economics focuses on the analysis of dilemma situations, which I will now turn to. According to Brennan and Buchanan [1985, 3ff.] and Tullock [1985, p.1079], a vast number of interactions can in principle be modelled as prisoners' dilemma situations.[23] In this way, it is possible to represent many potential conflicts between individuals and point to solutions which improve the situation of every actor [win-win-solutions, cf. Brennan/Buchanan 1985, p.3]. There are two possible settings: Dilemma situations may have positive (4.2.1) or negative consequences (4.2.2).

4.2.1 Positive consequences: The priority dilemma

The present institutional structure of science sets certain incentives for scientists. Among these are jobs (professorships), scientific reputation, and – probably to a lesser extent – money. These incentives force scientists into a *priority dilemma* that is similar to the situation of firms on ordinary markets. In this "winner-take-all-situation" competing research teams invest resources in solutions for the same problem [cf. Dasgupta/David 1994]. These teams would all be better off if either they worked on different problems or if they were not forced to be the first to find the solution, but (for example) only independently from other teams.

The relevant incentives in science, however, lead to a priority dilemma. But this is not something to be abolished. On the contrary, it is eventually superior to non-competitive solutions in providing society with knowledge. This has rarely been made clearer than by Friedrich August von Hayek [1978] in his general analysis of the benefits of competition. These benefits are:

1) Competition fosters innovation.

2) The successful pioneer forces others to imitate the successful strategy.
Both apply in the case of the priority dilemma. First, innovation, i.e., the construction of new theories, is fostered by the priority race. Second, though imitators do not benefit directly (e.g., in terms of reputation) from adopting the victorious theory, they cannot ignore this theory and must build on it in order to continue doing research in their field.

This shows that reconstructing a situation as a dilemma does not automatically mean that this situation is undesirable and should be overcome. A dilemma situation can only gain normative significance by other arguments, e.g. the con-

sensus of a scientific community's members.

I am now going to outline some applications of this dilemma:

1) The Devonian Controversy (cf. 4.1) illustrates the priority dilemma well: Rudwick's main protagonists compete fiercely over scientific reputation, government jobs, knighthood and financial rewards [cf. Rudwick 1985, 103ff.]. But it is this *interaction of competing individuals* that leads to a solution: As Rudwick [1985, 420ff.] persistently stresses, in the beginning none of the protagonists advocated or even considered the finally successful interpretation of the Devon. Instead, in the end this interpretation comprised ideas of nearly all scientists involved. Intense competition between the main actors thus promoted scientific progress: The solution of the controversy could not be found by a single actor, but only by *several* people, not working together but competing.

2) The idea of the priority race can also be found in David Hull's work, mainly on the history of evolutionary biology. In "Science as a Process" [1988], he writes:

> My goal in this book is to show that the coincidence between the professional interests of individual scientists to gain credit and the institutional goals of science to increase our knowledge of the empirical world is not in the least 'mysterious', nor the mechanism that produces this coincidence in the least 'hidden'. Science is so organized that once a person who is curious about nature gains entry into a particular scientific community and begins to receive credit for his or her contributions, the system of mutual use and checking motivated by self-interest comes into play. Science is so organized that self-interest promotes the greater good. [Hull 1988, p.357]

Thus, Hull equally emphasizes the productive role of striving for credit, as well as the idea that the organization of science directs the individual interests of each towards a "greater good" – Kitcher's idea of the invisible hand.

2) The "Nemesis Affair", concerning the (relatively recent) discovery of the remains of the asteroid which caused the extinction of the dinosaurs, has been documented by David Raup (1986). He writes that he decided to publish his first work on the subject in the *Proceedings of the National Academy of Sciences*, which allow for quick publication. According to Raup, though no one else was working on similar projects, at that time a lot of people had heard at least a little of his basic idea, and he was anxious that someone might dig out some old research project of his own dealing with the same idea. While he did not fear evil intentions and outright

stealing so much, he still knew from his own experience that it was hard to distinguish between own ideas and those overheard in conversations from colleagues.

So while it seems unlikely to Raup that anyone might 'steal' his priority, he nevertheless cannot completely exclude this possibility. At least he thinks it so important that he prefers a fast publication of his results. This hints at the fact that competition in Rudwick's scientific discipline is quite intense, forcing the actors to a priority race. The positive consequences of the dilemma can be seen in the many publications following Raup's work, which provided a huge lot of fruitful alternative hypotheses, confirmations and criticism.

4.2.2 Negative consequences: The property rights dilemma in science

The priority dilemma does not only have positive, but also negative consequences: Scientists may be induced to cheat or to steal from each other. In economic terms, this problem concerns the enforcement of property rights in science and can be reconstructed as a prisoners'dilemma:[24]

Suppose there are two actors X and Y, with utility functions comprising two arguments, direct utility and indirect utility.[25] So:

$U_{total} = f (U_{direct}, U_{indirect\ due\ to\ security\ of\ property\ rights})$.[26]

Also assume that X and Y can either work on their own or plagiarize/cheat/steal. Their pay-off-matrix looks like this:

		Y work on his own	plagiarize
X	work on his own	I B, B	II D, A
	plagiarize	III A, D	IV C, C

(where A > B > C > D)

Fig. 2: The dilemma of property rights in science

For both X and Y it is better to work on their own than to plagiarize. In this case, they gain indirect utility ($U_{indirect}$) due to the security of their property rights within their scientific community. At the same time, however, due to the incentives, both will commit theft, as the direct utility (U_{direct}) of stealing may be very high and may reduce the cost of one's own research significantly.

One single case of theft does not severely disturb the atmosphere in a scientific community. But if X commits theft, then Y is better off to steal as well in order to reduce his own costs and thus to compensate X's advantage. So both end up in the "social trap" of quadrant IV, although the situation in quadrant I would benefit them both. But both are afraid of being exploited by free riders (quadrant II and III).

Here point 3 of constitutional economics comes in, according to which a main purpose of economics is institutional design:

1) Several alternative institutions have evolved to overcome the scientific property rights dilemma.[27] First, the scientific public plays a major role here. If stealing is made public, this might deter possible future plagiarists. In the Devonian Controversy, when one actor sees his property rights in danger, he threatens the president of the Geological Society, "if you do not interpolate our names, my honest opinion is that you will do yourself a disservice & be sorry for it hereafter" [Rudwick 1985, p.346]. In other words, he threatens to make the controversy public and thereby damage the reputation of geology as a whole. In his day, this solution worked. In later times, more formal institutions are required. This includes, for example, patents and specific mechanisms of submitting papers to scientific journals.

2) In Hull's work, another case of endangered scientific property rights (and reputation) can be made out: Hull [1988, 46f.] mentions a debate that originated in 1863 from the publication of Charles Lyell's "The Geological Evidences of the Antiquity of Man" and involved prominent biologists like Ch. Darwin, T. H. Huxley, and J. D. Hooker. In his book, Lyell listed a lot of evidence for the descent of man without adequately referring to their discoverers:

> The ensuing controversies over inadequate acknowledgement among allies and former friends were even more acrimonious than the dispute with Owen.[28] Darwin, Hooker, and Huxley were caught in the middle. *No matter which way they went, they would lose professional friends and allies, and the cause of evolutionary theory would suffer.* [Hull 1988, 46f.; my italics]

In this passage, it become clear that neglecting property rights (i.e., priority rights)

may bring about many disadvantages for all involved and result in a dilemma situation. Without explicitly using the term 'prisoners' dilemma', Hull [1988, p.520] asks: „How prevalent can stealing become before the likelihood that one will get credit for one's contributions is so reduced that the system ceases to work?" For him, the main way to solve this problem is to establish a *peer review* system [cf. Hull 1988, 394f].

3) Moreover, specific institutional frameworks could make productive use of scientists' different starting positions (e.g. in resources or career expectations) in order to achieve the desired distribution of risk. So it could make sense to set incentives for unemployed scientists that enable them to work on more exotic, rather neglected projects. In this way, advice from game theory could be implemented like a management-consultant's advice to an enterprise: Find out which theory is – under the given constraints – optimal for you (in the long run)!

5) Conclusion: Science saved?

As I have tried to make clear, constitutional economics makes the application of the economic approach to philosophy of science especially fruitful. For the question of scientific rationality, the following consequences can now be drawn:

1) Constitutional economics abandons more traditional welfare theory and instead builds on different levels of consensus as a normative foundation. Most criticisms against the economic argument for scientific rationality can be answered in this way. E.g., a methodologically sound conception of the 'social' can now be presented.

2) Kitcher's invisible hand argument against relativist science studies works only within the adequate institutional framework. This could be analyzed further, by looking at particular dilemma situations and institutions which shape science. Two examples of this have been briefly sketched in this paper.

3) Economics of science is a field different from science studies, sociology or history of science. Unlike them, economics of science is meant to be relevant for the *normative* aspect of science, a classic task of philosophy of science. Science studies, history and sociology of science provide the empirical basis which the philosopher of science can use for economic reconstructions, but they are not the whole story. Thus, philosophy of science could integrate the economic task of institutional design also into its *normative* framework, something it could rather not learn from science studies.

Notes

[1] Cf. Barnes [1974], Bloor [1991].

[2] Cf. Kitcher [1993], Hands [2001], Rescher [1978], [1989] and [1996], and Wible [1998].

[3] Cf. Kitcher [1993] and [2001].

[4] For the difference, cf. Hands [1994].

[5] Cf. Barnes [1974] or Pickering [1984].

[6] Cf. for example Furubotn/Richter [1992].

[7] For an overview of related approaches in economics and sociology of science, cf. Diamond [1988], Dasgupta/David [1994], Stephan [1996], and Wible [1998].

[8] "[T]he operation of social systems in ways that we might initially view as opposed to the growth of knowledge can be dependent on the use of complicated reasoning and can contribute to the community's attainment of its epistemic ends." [Kitcher 1993, p.388].

[9] Brock and Durlauf [1999] have developed Kitcher's argument further. They show that external factors do not necessarily have consequences for scientific progress. Under relatively weak conditions, external factors can accelerate and strengthen the emergence of a consensus in a scientific community.

[10] For a more detailed treatment, cf. Lütge [2001, ch. 1.2.5].

[11] This applies also to the related approaches of Larry Laudan [cf. Hands 1996] and Alvin Goldman [cf. Sent 1997].

[12] Similarly, Laudan [1987, 1990 and 1996] wants to reconstruct "our goals [of science, C.L.]" in order to gain a criterion for evaluating the progress of research traditions.

[13] Actually, Mirowski's argument can already be found in Boland [1971].

[14] Cf. already Arrow [1951].

[15] Recently, Kitcher [2001] has modified his view: He no longer tries to find a single aim for science. However, his other main arguments remain intact. Cf. also Longino [2002].

[16] To my knowledge, Zamora Bonilla [2002] is the only case.

[17] It is interesting to see that most economic approaches in philosophy of science focus on this 'lone decision maker'.

[18] Kitcher [1993, 217] and Martin Rudwick [1985, 420ff.] hold this view.

[19] Buchanan illustrates Hobbes'anarchy by the prisoners'dilemma, cf. Buchanan [1975, ch. 1].

[20] I believe that even most extremist positions – that are often taken to imply that the existence of science is a fundamental evil in itself – only fight *certain* forms of science, like the western one.

[21] Cf. also Wible [1998].

[22] Martin Rudwick's "Great Devonian Controversy" [Rudwick 1985] is the perfect example, as it provides a very detailed narrative and gives deep insights into the factors influencing the actors'theoretical decisions. This Controversy took place during the 1830s and 1840s, when the "Devonian" was accepted as a new geological period. For reasons of limited space, I cannot go into detail about this controversy here. Cf. Lütge [2001, ch. 3.4].

[23] Especially in his new work [2001], Kitcher himself hints at the existence of dilemma situations [e.g., chs. 7ff.], but does not make systematic (economic) use of them. Cf. also Longino [2002].

[24] Wible (1992] has already treated the problem of fraud in science, though considerably different.

[25] Note that, according to constitutional economics, utility comprises all advantages an actor *regards* as advantages, like fame, status, power, and so on. – Concentrating on direct and indirect utility is, of course, a simplifying assumption for the problem at hand. Amore detailed reconstruction would have to take more factors into account.

[26] This is to say that the actors profit from a common atmosphere in which they all respect each other's property rights. This reduces everyone's need to invest in security mechanisms (such as secrecy).

[27] In game theoretic terms, overcoming the trap would mean to change the pay-off matrix by making the option of defecting more expensive, e.g. by constructing or reforming the institutional framework.

[28] Unlike Darwin, Darwin, Lyell, Hooker, and Huxley, Richard Owen advocated an idealist view of evolution, while at the same time claiming priority for Darwin's evolutionary ideas from the "Origin of Species" [cf. Hull 1988, pp.43-46].

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